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SATURDAY, NOVEMBER 26, 1881.

THE SATELLITES OF MARS.

The outer satellite of Mars was seen here on Nov. 15th, and by an observation of Nov. 20th its position was

WASHINGTON, M. T.

		h.	m.	p.	s.
1881.	Nov. 20,	13	15,	71°.7,	45".6.

This satellite is therefore near the predicted place. An hour later Phobos seemed to be visible, also near the computed position, but the sky had become a little thick and I could not be certain of seeing this satellite.

The planet will continue to approach the earth until December 21, and the satellites will become brighter. It is possible, therefore, that they may be observed for nearly two months during the present opposition.

A. Hall.

Washington, D. C., *Nov.* 22, 1881.

THEORY OF THE MOON'S MOTION.*

About a year ago the Vice-President of the Physical Section of our chief scientific association remarked, in his farewell address: "there are many subjects in astronomy that need investigation, but in most cases the labor required is very great, and the completion of the work would occupy a long time. * * * The lunar theory has been a vexed question for the last two centuries, and may remain so for a long time to come." If persistent, painstaking, and conscientious effort have aught to do with such a matter, we must add to the list of distinguished lunar theorists, including Plana, Damoiseau, Hansen, and Delaunay, the name of Stockwell. We cannot say that his researches have yet met with that notice to which they are perhaps rightly entitled. Mr. Stockwell has published a number of monographs on many points connected with the lunar theory during the last six or seven years; and his works show great familiarity with, and expertness in, the involved computations of this sort of astronomical research.

If we may judge from the appearance of the pamphlet before us, Mr. Stockwell has now quite terminated his lunar investigations, and intends to complete the publication of his finished theory of the moon's motion at some early date. In his Introduction he has sketched the early historic development of the question with that explicitness which we should expect rather to have seen in some thorough elementary text-book; strangely, he devotes twice as much space to the ante-Newtonian aspect of the problem as to the most remarkable developments of the mathematical theory which have occurred since his time. He makes no mention of Damoiseau, who takes high rank not only among pure lunar theorists, but among the constructors of tables of the moon. His tables are well known to have been the first ever constructed from pure theory.

Though the age of the great lunar investigators is now gone, there are some very surprising results of Mr. Stockwell's "new method of analysis" to which the attention of the few theorists now working at the moon's motion might well be directed. He instances several comparisons of the values of his co-efficients with those obtained by Delaunay in his very refined development; in one case he obtains, by a rapidly-converging series of four terms, a result identically the same with that of Delaunay's series of seven terms; and remarks, "the four terms of my development are more accurate than the seven terms of Delaunay's, since the seventh term of the latter series is thirty times greater than the fourth term of the former." There is nothing new in the fact that the sum of a very small number of terms should come out equal to a very large series, but if theorists can be brought to acknowledge the essential accuracy of the "new method," Mr. Stockwell must no doubt be credited with effecting an enormous advance in mathematical astronomy. Mr. Stockwell has shown satisfactorily to himself the correctness and value of his method, and the facility of its application—he must now address himself to the equally difficult task of making others see it in the same light.

It seems a wholesale assertion on the part of Mr. Stockwell that there are "several terms of considerable magnitude in the theories of La Place, Plana, Pontécoulant and Delaunay, which are not functions of the disturbing force;" and we should, at first blush, be inclined to place much confidence in his demonstration that the general integral assumes the indeterminate form in special cases which occur in those theories. It is certainly a most important oversight, and leads us to believe that the lunar theorists who followed La Place would have done much better to have built up theories of their own with entire independence of what anyone else had done. It is a remarkable fact if this discovery has been left for Mr. Stockwell to make. He concludes: "if the computations of the present work are correct, astronomers have carried their approximations to terms of the *fifth*,

* John N. Stockwell, Ph. D. (Introductory.)

sixth, and *seventh* orders of magnitude, before those of the *third* and *fourth* had been correctly computed. This seems to be a sufficient reason for the nearly stationary condition of the lunar theory during the past three-quarters of a century, notwithstanding the great efforts which have been made to perfect its solution. Its advancement has been blocked by the obstacles thrown in its path by analysis itself; and we may therefore reasonably hope for substantial improvement in the theory and tables when they are no longer embarrassed with equations which have no existence in nature."

We may remark that there are two ways in which the correctness of Mr. Stockwell's conclusions may be tested: first, a mathematical expert competent to pronounce upon his theoretic processes should go over his work with the most searching criticism in every detail; and second, his theory should be compared with observations. But this latter would be a task of such immensity that no astronomer unassisted would hope for its completion.

NEW YORK ACADEMY OF SCIENCES.

Oct. 31, 1881.

The President, Dr. J. S. Newberry, in the Chair. Twenty persons present.

The following paper was read by Mr. John H. Furman:

"The Geology of the Copper Region of Northern Texas and the Indian Territory."

The well-marked cretaceous beds of Parker County, Texas, extend for 30 miles north of west from Weatherford, on the road to Graham. They consist of strata of shelly limestone, sandstone and shaly clay, the latter grayish or reddish in color. An occasional thin seam of soft coal is found; and the water is strongly impregnated with lime. A stratum of sandstone stretches for thirty miles N. W. from Fort Worth. In this rock springs are found containing sodic carbonate, similar to the waters of the artesian wells of Fort Worth, Tarrant County, at a depth of about 270 feet. Towards Graham, the country assumes a semi-mountainous appearance, and, for twenty-five miles or more, sandstone ridges alternate with prairies, the hills being covered with scrub oak. Some of the ridges attain an elevation of two or three hundred feet above the prairies. The strata are horizontal, and large portions of the original surface have been carried away by erosion. The upper stratum is in many places a conglomerate, made up of small pebbles. In this region the seams of coal met with are generally soft, and the only workable bed known is one about three feet thick, yielding a fair quality of bituminous coal, which crops out and has been traced for several miles near the Clear Fork of the Brazos river in Young County. This supposed coal region has a general N. E. and S. W. direction.

Approaching Graham the prairies begin to resemble the plains; and the ridges, capped with sandstone, show bases of mottled reddish-colored shales, or clay; salt springs and salt streams are found, indicating the border of the great alkaline region. From Graham to Fort Griffin in Shackleford County, thence north in Throckmorton County, the country rises. Every few miles a steppe is mounted, the face of the escarpments showing horizontal thin limestone strata. The same features continue, and then the country slopes towards the Brazos river.

Turning westward through Haskell County, the surface lowers again towards the Brazos, the river coursing south to north, and a plain is crossed, the ground differing from any observed. The soil is mixed and covered with gravel, in many places several feet deep. The pebbles vary in size from half an inch to an inch and a half in diameter, and consist of feldspar quartz, porphyry, and basalt. On the western side of Haskell County the copper bed is reached not far from the Brazos river; and west of the copper a great belt of gypsum hills, several miles in width, extends northward, parallel with the copper bed, into the Indian Territory. Gypsum occurs there in most of its forms, including selenite which has been locally mistaken for mica.

On reaching a scene of attempted mining operations in search of supposed veins of copper, a very short examination convinced me that no vein would ever be discovered. Denudation has laid the bed bare, sweeping away the larger portion uncovered and leaving only patches; but these were sufficient to give a clear conception of the mode of occurrence. The copper-bearing stratum is an ashy-colored clay shale, more or less tinged with green, the upper portion showing the deep green carbonate of copper, usually two or three inches thick. Overlying this stratum is a cap-rock of gypsiferous sandstone, about three feet thick, with a layer ⅛ to ¼ inch thick, impregnated with carbonate of copper, as though it had soaked it up from below. Underneath the gray or green bed an intensely red clay shale is generally found. Nuggets of copper are scattered over the surface of the red bed, with pieces of cuprified wood and nuggets of iron pyrites. In the wood the original structure in many instances is perfectly preserved, also appearing cuprified in all stages of decay, as though it had become half rotten before the petrifaction was effected. The overlying sandstone frequently contains biscuit-like concretions of gypsum. Juniper trees abound and also cover the gypsum hills, the perfectly preserved cuprified wood, with its knots and bark, showing a fac-simile of that growth. I found in the gray bed fragments of wood partially unaltered, as though it had just commenced to absorb copper; also large pieces of coal, three or four inches or more in diameter, the cracks of the same piece being filled with crystalline carbonate of copper, or with white gypsum, thus appearing veined with copper and gypsum. In parts of the bed remaining the resemblance to piles of ashes and charcoal is strikingly deceptive; in one shaft, sunk to a depth of about thirty feet, the horizontal position of the strata was confirmed, the shaft passing through the cupriferous gray bed, and then through a succession of layers of red shale and soft red sandstone, in which not a trace of copper was found. The gray stratum extends seventy-five feet or more under a point of the gypsum hill. In a tunnel traversing this stratum I noticed occasionally pebbles belonging to the gravel drift. This copper formation has a general north and south course, usually less than fifty yards in width, and was traced for a distance of eight or ten miles to the southern boundary of Haskell County.

At one point the gray bed lies between beds of sandstone; the red bed does not appear, and the underlying sandstone strata are almost white, laminated, and very hard. The bed is more than two miles distant from the gypsum hills; the gravel drift is noticeable and even abundant. Observing the nuggets of copper ore and the drift pebbles lying about in places on the red bed, the idea forced itself upon me that there might be a remote connection between the two. However, the nuggets of ore are evidently concretions, and no pebbles occur in the gray bed. The gypsum range extends several miles across, with a western declivity similar to that on the eastern side. A plain, a little over one hundred feet below, reaches beyond to the foot of the great Llano Estacado. On these hills and on this western plain the gravel drift is wanting.

The copper bed was traced five miles further to the north; also in Knox county, not far from the Wichita river, and forty miles or more north of the southern portion of Haskell county, besides learning its supposed occurrence north of the Wichita river. The copper band

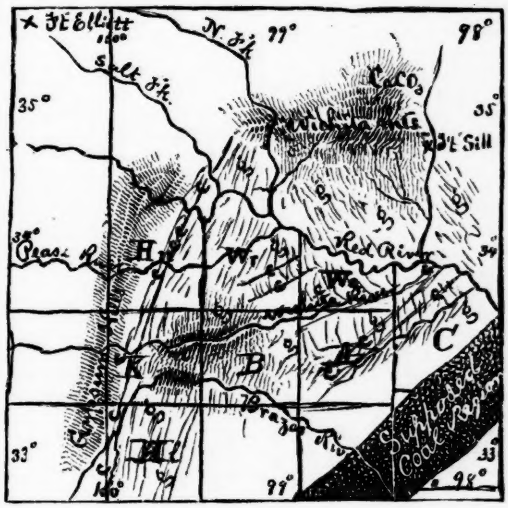
here lies between the sandstone and gray bed, with the red beds beneath. Eastward, between the Brazos and Wichita rivers, the gravel drift is abundant, with many stones of greater diameter. At the "Narrows," between the Wichita and Brazos rivers, the width is only sufficient to admit the passage of a single wagon. Continued caving in of the bluffs of the two rivers has widened an immense eroded area, rendering a large surface valueless, and while the channels of the rivers are several miles apart, their junction is only a question of time. In the copper region of the little Wichita river, near the centre of Archer county, the ore occurs under the same general conditions, with a different course, N. E. and S. W. and copper nuggets, coal and cuprified wood are found.

Embedded in the overlying sandstone, in some instances several feet above the gray bed, the sandstone frequently attains a thickness of more than fifteen feet. The cuprified wood is altogether different from that of Haskell county, and resembles the wood of the mesquite tree, which I found scattered about. The gravel drift here is identical in character with that of the region further west, and pebbles occur in the gray copper-bearing bed beneath the sandstone. The extension of the gravel drift of Haskell county, beyond the Brazos river system, its absence west of the gypsum hills, the larger size of the pebbles in Knox county, bordering the Wichita river, and the occurrence of the drift only in the vicinity of the copper-bearing lines mentioned, and in Archer county, suggested to me a possible relationship of some kind between the two, perhaps their origination in the same region.

Between the Wichita and Pease Rivers I crossed several copper-bearing beds, having a general northeast and southwest direction. In Wilbarger County the gravel drift is in great quantity, and boulders from three to seven inches in diameter occur. In places, and having a northeast and southwest bearing, heavy deposits or lines of gravel and boulders attract attention, appearing as though a great flow towards the southeast had met obstructions along its course, the great incline of this region being directed toward the southeast. Beyond Pease River the gravel drift lessens, but the large boulders are occasionally seen as far west as the gypsum hills. Not far north from the centre of Hardeman County I again found the Haskell County copper bed, the accompanying sandstones being thin and much mixed with gypsum. The copper bed reaches higher than the surrounding country, except the gypsum hills to the west. From this high locality of the copper, known as Prairie-dog Mounds, the country inclines on one side northward to a creek emptying into Red River, and on the other side southward to the Pease River.

South of these mounds, where only here and there patches of the bed are preserved in the midst of a general erosion, I found the largest mass of copper ore thus far discovered, consisting of an aggregation of cuprified wood, resembling the trunk of a tree, more than one foot in diameter. Beyond Red River the bed continues to the vicinity of the Salt Fork of Red River, distant but little over 20 miles from the Wichita Mountains of the Indian Territory. The bed probably continues nearly to the western end of these mountains, and here must be found the true centre of elevation and the origin of the gravel drift. The Haskell County copper bed was also traced south to the Wichita River, thus establishing its continuity from the southern portion of Haskell County, through Knox and Hardeman Counties, into the Indian Territory, a length of more than 100 miles. Subsequently, the northern end of the bed was found a short distance from the western end of the Wichita Mountains, on the south side of the range. The copper formations of Archer and Wichita Counties continue through Clay County to the Red River boundary of the Indian Territory. The gravel drift does not extend to the north of the Wichita Mountains, but a limestone district occurs about 20 miles in width, that reaches probably as far out to the north, from the Wichita Range, the course of the latter being east and west. This limestone area may be called mountainous, is much disturbed and tilted, and is similar in appearance to the metalliferous limestone formation of Mexico. The Wichita Mountains are mainly made up of porphyries, trachytes and basalt, and appear to be two parallel ranges with transverse ranges and small valleys between. About 12 miles west of Fort Sill an extensive body of hornblende slate makes its appearance between the two main ranges. The drift from the mountains extends to the south and southeast. It is found as far west as the Haskell County copper bed, and as far east as the Archer County copper bed is known. The river channels of that section of the country have been formed since this drift period. The development of the Wichita Mountains seems to have marked the close of a period of uplift and simultaneous erosion.

These mountains have the same general appearance as the Rocky Mountains, which pass through the western portion of Texas and the State of Coahuila, Mexico; and it has been a matter of much interest to observe that similar drifts of local origin are frequently met in the latter regions. The Wichita Mountains appear to be identical in origin with the Rocky Mountains, and constitute the most eastern spur of that system. In Northern Mexico short ranges are encountered, striking east and west, and of these the Wichita Mountains appear to be a reproduction. The Wichita Mountains will be found to contain mineral deposits, possibly of some value; veins of copper ores do exist 40 miles west of Fort Sill, near Otter Creek, in the mountains; but I am convinced that the copper bed or stratum of Northern Texas will prove of no commercial importance.



SCALE—52 MILES TO 1 INCH.

A. Archer County.
B. Baylor County.
C. Clay County.
Hl. Haskell County.
Hn. Hardeman County.
Wa. Wichita County.
Wr. Wilbarger County.
c. c. c. Copper Bed.
g. g. g. Gravel Drift.
n. Narrows.

Prof. Newberry remarked that the communication of Mr. Furman was of great interest, since no accurate description had before been given of the geological

structure of the region where the copper occurs in northern Texas and the Indian Territory. He had received specimens from that region long ago and recognized their similarity to the copper ores of New Mexico, where in the upper portion of the Triassic formation copper forming concretions and replacing wood occur in many localities, and have been more or less mined for. In one locality near Abiquini very extensive galleries have been cut in the sandstone in search of copper which there replaces branches and trunks of trees and forms concretions which are irregularly scattered through the rock. Here the work was done by the early Spanish explorers perhaps 200 years ago, and the remains of the furnaces in which the copper was smelted are still to be seen at the mouth of the mine. Still further west, in southern Utah, the same formation carries copper and considerable silver, at Silver Reef enough to pay well for mining, but in no locality yet known are the deposits of copper ore sufficiently concentrated and continuous to make mining for that material profitable, so it would doubtless be found in Texas and the Indian Territory. The copper was deposited with the Triassic rocks from a shallow sea in which an unusual quantity of copper was held in solution. This impregnated the sediments found at the bottom replacing wood and forming as nodules about some nucleus. The aggregate quantity of copper in this formation was enormous, but, except where by the erosion of the beds it accumulated at the surface and could be picked up without any expense in mining, it would hardly pay to attempt to obtain it by ordinary mining processes.

The wood replaced by copper Dr. Newberry said was undoubtedly all coniferous, and different from any now living. The beds which contained the cuprified wood also contained much that was silicified. Of this he had examined many specimens under the microscope and had found the peculiar dotted cells which are characteristic of the coniferæ, and these grouped in such a way as to prove the trees to have belonged to the Araucarian group of conifers. So far as yet known the angiosperms, or higher order of plants, did not make their appearance on the earth's surface until after the copper bearing rocks of the southwest had been deposited.

THE AMERICAN CHEMICAL SOCIETY.

The November meeting of this Society was held on Friday evening, November 4th, with Vice-President Leeds in the Chair.

The following gentlemen were duly elected members: Dr. C. W. Volney, Dr. Witthaus, Messrs. C. E. Munsell, W. W. Share, J. D. O'Connor, and Day. The first paper of the evening was "On some New Salts of Thymole Sulpho-acid, and some new facts concerning the same," (a second paper) by Mr. J. H. Stebbins, Jr., S. B. The sodium salt having the formula

$$C_6H_2\,(CH_3)\,(C_3H_7)\,(NaSO_3)\,O\,Na+2\tfrac{1}{2}H_2O$$

was described, and also the free sulphur salt had its characteristics enumerated.

Mr. Stebbins followed with a second paper "On the Combination of Diazo Compounds with Thymole Sulpho-Acid."

In this he described the experiments which he performed in his work, the results of which were given in the first paper. Both were technical and not of any popular interest.

The third paper was by Dr. C. W. Volney, and was entitled, "The Constitution of the Explosive Derivatives of Glycerine."

In this communication the author tried to prove that the nitro-glycerine was composed by the substitution of the nitrogen trioxide (NO_3) instead of the nitrous oxide NO_2, making the formula $C_3H_5\,(NO_3)_3$ instead of $C_3H_5\,(NO_2)\,O_3$, and secondly, he showed how it was possible to substitute chlorine for the nitrogen trioxide and so produce a new explosive compound.

This paper provoked much discussion on account of the theoretical arrangement of the atoms necessary to sustain Dr. Volney's statement.

Subsequently the Committee on Nominations reported that the following ticket was recommended to the Society for their votes at the December meeting.

Corresponding Secretary.— P. Casamajor.
Recording Secretary.—J. H. Stebbins, Jr.
Treasurer.—M. Alsberg.
Librarian.—Geo. A. Prochazka.
Curators.—A. J. Rossi, Wm. Rupp, A. A. Fesquet.
Committee on Publications.—Arno Behr, A. R. Ledoux, H. Endemann.
Committee on Nominations.—A. H. Elliott, O. H. Krause, J. P. Battershall, J. B. F. Herrishoff, T. O'C. Sloane.
Board of Directors.—P. Casamajor, J. H, Stebbins, Jr., H. Morton, C. F. Chandler, M. Alsberg, E. R. Squibb, W. H. Nichols, W. H. Habershaw, E. Waller, A. H. Gallatin, Geo, A. Prochazka.

ON THE NATURE OF THE DIPHTHERITIC CONTAGIUM.

By Dr. H. C. Wood.

The lecturer began by stating that the researches which formed the basis of the present address had been made under the auspices, and, indeed, at the suggestion, of the National Board of Health, by Dr. Henry F. Formad and himself, who were jointly responsible for the facts and inductions and jointly deserving of whatever reprobation or approbation might be due. The full text of the work is now in the hands of the National Board, and will be shortly published by them as an appendix to their annual report, and the lecturer desired that criticism be withheld until this was done, as the memoir will contain much that cannot be spoken of in the present lecture.

In the spring of 1880 work was begun by inoculating rabbits with diphtheritic membrane taken from the throats of patients at Philadelphia. An account of the labors of the following summer has been already published, but it seems necessary to epitomize them here. It was found that only in a very few cases was anything like diphtheria produced in the rabbit by inoculating with the membrane. The inoculations were practised by putting pieces of the material sometimes under the skin, sometimes deep in the muscles. Many rabbits died after some weeks, not of diphtheria, but of tuberculosis. In a series of experiments it was shown that this tuberculosis was an indirect and not a direct result of the inoculation, and that any apparent relation between the two diseases is only apparent, not real. Next, the tracheas of a series of rabbits were opened and false membrane inserted. It was found that under these circumstances a severe trachitis was frequently produced, and was attended by an abundant formation of pseudo-membrane. Careful studies made of the false membrane of diphtheria and of this false membrane showed that the two were identical, both containing in abundance fibrin fibres, corpuscular elements, and various forms of micrococci. To determine whether other inflammations of the trachea than that caused by diphtheria or its membrane are accompanied by the formation of false membrane, a number of experiments were made, and it was demonstrated that the production of false membrane has nothing specific in it, but that any trachitis of sufficient severity is accompanied by this product. Careful studies also showed that this false membrane does not differ in its constitution from that of true diphtheria, except it be that the micrococci are not so abundant in it. We always found some micrococci, and in some of these traumatic pseudo-mem-

*An address made before the Academy of Natural Sciences.

branes they were almost as numerous as in the diphtheritic exudation.

Last spring we resumed our investigations. Having heard that there was a very severe epidemic in Ludington, Mich., Dr. Formad was dispatched to examine cases and collect material. He found a small town situated upon the shore of Lake Michigan, in the centre of the lumber region, with inhabitants mostly engaged in the lumber trade and in managing very numerous large saw-mills. The town was all built upon high ground except the Third Ward. This occupied a low swamp which had been filled in largely with sawdust. The soil was so moist that a hole dug in it would fill at once with water, and but few houses had any attempts at cellars. It was in this district that the disease had prevailed. Almost all the children had had it, and one-third of them were said to have died. Dr. Formad examined a large number of cases, obtained a supply of diphtheritic membrane, and brought home pieces of the internal organs of a child upon whom he had made an autopsy. In every case the blood was found more or less full of micrococci, some free, others in zooglœa masses, others in the white blood-corpuscles. The organs brought home also all contained micrococci, which were especially abundant in the kidneys, where they formed numerous thrombi, choking up and distending the blood-vessels. In the summer of 1880 we examined the blood of several cases of endemic Philadelphia diphtheria, and in no case found any new elements in it. But during the present summer we have found micrococci in the blood of Philadelphia diphtheritic patients, showing the differences in the disease are simply in degree, not in kind.

Experiments were now made with the Ludington material upon animals. Inoculations were practised under the skin, deep in the muscles, and in the trachea. In all cases the results were similar. A grayish exudation appeared at the seat of inoculation, along with much local inflammation, the animal sickened, and in the course of a few days death occurred. The local symptoms increased and widened. In some cases the false membrane spread from where the poison had been put in the trachea up to the mouth. The blood examined during life or after death was found to contain micrococci precisely similar to those found in the Ludington cases, and in a few instances micrococci were found in abundance in the internal organs. Studies made upon the blood of these animals, as well as upon the Ludington cases, show that the micrococci first attack the white blood-corpuscles, in which they move with a vibratile motion. Under their influence the corpuscles alter their appearances, losing their granulations. They finally become full of the micrococci, which now are quiescent, and increase until the corpuscle bursts and the contents escape as an irregular, transparent mass full of micrococci, and form the so-called zooglœa masses. In the diphtheritic membrane the micrococci exist frequently in balls, and it is plain that these collections are merely leucocytes full of the plant. The bone-marrow of the animals were found full of leucocytes and cells containing micrococci.

The question now arose, is the disease produced by diphtheritic inoculation in the rabbit diphtheria? We concluded that it is, because the poison producing it is the same, the symptoms manifested during life are the same, and the post-mortem lesions are identical. The contagious character of the disease is retained, as we succeeded in passing it from rabbit to rabbit.

Our next series of experiments were directed to determining whether the micrococci are or are not the cause of the affection. The experiments of Curtis and Satterthwaite, of New York, have shown that the infectious character of diphtheria depends upon its solid particles; for when they filtered an infusion of the membrane it became less and less toxic in proportion as the filtration was more and more perfect; and when the infusion was filtered through clay, the filtrate was harmless.

The urine of patients suffering from malignant diphtheria is full of micrococci, and may contain no other solid material. Following the experiments of Letzerich, we filtered this urine and then dried the filter-paper. Upon experiment we found this even more deadly in its effects than is the membrane. The symptoms and lesions following in the rabbit inoculation with such paper are precisely those which would have ensued had a piece of diphtheritic kidney or membrane been employed. This experiment shows that the solid particles of the membrane, which are the essential poison of malignant diphtheria, are the micrococci, which must be either the poison itself or the carriers or producers of the poison.

Leaving for a while this point, I will next direct your attention to our culture-experiments. These were performed in the manner commenced by Klein and that recommended by Sternberg. The first method seems to us the best for the purpose of studying the development of the micrococcus itself; the second the best for the obtaining of it in quantity for experimentation.

We cultivated micrococci from the surface of ordinary sore throats, from furred tongue, from cases of mild diphtheria as we commonly see it in Philadelphia and from Ludington cases. We found, in the first place, that there were no differences to be detected in the general or special appearance of the various micrococci, and no constant differences in size. We found that they all formed similar shapes in the culture apparatus; they had this difference, however,—whilst the Ludington micrococci grew most rapidly and eagerly generation after generation up to the tenth, those from Philadelphia diphtheria ceased their growth in the fourth or fifth generation, whilst those taken from furred tongue never got beyond the third transplantation. Various culture-fluids were used, but the results were identical. We conclude, therefore, that as no difference is detectable between the micrococci found in ordinary sore throat and those of diphtheria, save only in their reproductive activity, they are the same organisms in different states. As the result of some hundreds of cultures, we believe that the vitality of the micrococci under artificial culture is in direct proportion to the contagious powers of the membrane from which they have been taken. We have made many inoculations with cultivated micrococci and have succeeded in producing diphtheria with the second generation, but never with any later product. This success, taken in conjunction with the urine experiments already spoken of, seems to us sufficient to establish the fact that the micrococci are the *fons et origo mali* of diphtheria. The experiments of Pasteur and others have proven that it is possible for an inert organism to be changed into one possessed of most virulent activity, or *vice versa*, and we believe that we can offer direct proof that the micrococci of the mouth are really identical in species with the micrococci of diphtheria, and do not merely seem to be so. We exposed the Ludington membrane for some weeks to the air in a dried condition. There was no putridity or other change detectable in it; but, whereas formerly it had been most virulent, now it was inert, and its micrococci not only looked like those taken from an ordinary angina, but acted like them. They were not dead, they had still power of multiplication, but they no longer grew in the culture-fluid beyond the third or fourth generation. Certainly they were specifically the same as they had been, and certainly therefore the power of rapid growth in culture-fluids and in the body of the rabbit is not a specific character of the diphtheria micrococcus.

As is well known, Pasteur attributes the change from an active to an inert organism to the influence of the oxygen of the air upon the organism. Whether this be true or not of the diphtheria micrococcus is uncertain, but the effects of exposure of the dried membrane seem to point in such direction.

With the facts that are known in regard to the clinical history of diphtheria and those which we have determined in our research, it is easy to make out a theory of the disease which reconciles all existing differences of opinion and seems to be true.

A child gets a catarrhal angina or trachitis. Under the stimulation of the inflammation products the inert micrococci in the mouth begin to grow; and, if the conditions be favorable, the sluggish plant may be finally transformed into an active organism, and a self-generated diphtheria results. It may be, however, that by appropriate treatment such a case is arrested before it fairly passes the bounds of an ordinary sore throat. Every practitioner knows that such diversity does exist. Again, conditions outside of the body favoring the passage of inert into active micrococci may exist, and the air at last become well loaded with organisms, which, alighting upon the tender throats of children, may begin to grow and themselves produce violent angina, trachitis, and finally fatal diphtheria.

In the first instance we have endemic diphtheria as we see it in Philadelphia; in the second, the malignant epidemic form of the disease as it existed in Ludington. It is also apparent that in the endemic cases the plant whose activity has been developed within the patient may escape with the breath, and a second case of diphtheria be produced by contagion. It is also plain that as the plant gradually in such a case passes from the mild to the active state, there must be degrees of activity in the contagium, one case being more apt to give the disease than is another; also that the malignant diphtheria must be more contagious than the mild endemic cases. We think there is scarcely a practitioner who will not agree that clinical experience is in accord with these logical deductions from our experimentally determined premises.

It yet remains for us to investigate as to what are the conditions outside of the body which will especially favor the production of active micrococci, and also to study the effects of agents in killing these organisms; for it is very apparent that local treatment of the throat must often be of the utmost importance, and that it will be far more effective if it be of such character as to kill the micrococci, and not simply be anti-phlogistic in its action.

SOLAR PARALLAX.

In an elaborate paper, given in full in the *American Journal of Science*, for November, Professor William Harkness draws the following conclusions:—

For convenience of reference the limiting values of the solar parallax, found by the various methods described in the foregoing pages, are presented here. It should be remarked, however, that in selecting these values the results of all discussions made prior to 1857 have been omitted; except in the case of the transit of 1761, and the smaller of the two values from the transit of 1769.

I.—Trigonometrical methods.

Mars, meridian observations	8".84 —	8".96
" diurnal observations	8.60 —	8.79
Asteroids	8.76 —	8.88
Transit of Venus, 1761	8.49 —	10.10
" " 1769	8.55 —	8.91
" " 1874	8.76 —	8.85

II.—Gravitational methods.

Mass of the earth	8".87 ±	0".07
Parallactic Inequality	8.78 —	8.91
Lunar Inequality	8.66 —	9.07

III.—Photo-tachymetrical methods.

Velocity and light equation	8".72 —	8".89
Velocity and Aberration	8.73 —	8.90

To obtain a definite value of the solar parallax, it would now be necessary to form equations of condition embodying the relations between the various elements involved; to weight these equations; and to solve for it by the method of least squares. But what is the use? It is perfectly evident that by adopting suitable weights, almost any value from 8".8 to 8".9 could be obtained, and no matter what the result actually was, it would always be open to a suspicion of having been cooked in the weighting. We only know that the parallax seems to lie between 8".75 and 8".90, and is probably about 8".85. Attack the problem as we will, the results cluster around this central value. All the methods give a probable error of about ±0".06, and no one of them seems to possess decided superiority over the others. We have nearly exhausted the powers of our instruments, and further advance can only be made at the cost of excessive labor.

In the beginning of the eighteenth century the uncertainty of the solar parallax was fully two seconds; now it is only about 0".15. To narrow it still further, we require a better knowledge of the masses of the earth and moon, of the moon's parallactic inequality, of the lunar equation of the earth, of the constants of nutation and aberration, of the velocity of light, and of the light equation. All these investigations can be carried on at any time, but there are others equally important which can only be prosecuted when the planets come into the requisite positions. Among the latter are observations of Mars when in opposition at its least distance from the earth, and transits of Venus.

In 1874 all astronomers hoped and believed that the transit of Venus which occurred in December of that year would give the solar parallax within 0".01. These hopes were doomed to disappointment, and now, when we are approaching the second transit of the pair, there is less enthusiasm than there was eight years ago. Nevertheless the astronomers of the twentieth century will not hold us guiltless if we neglect in any respect the transit of 1882. Observations of contacts will doubtless be made in abundance, but our efforts should not cease with them. We have seen that the probable error of a contact observation is ±0".15, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. On the other hand, the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only ±0".55. If the sun is visible for so much as fifteen minutes during the whole transit, thirty-two negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. In view of these facts, can it be doubted that the photographic method offers as much accuracy as the contact method, and many more chances of success?

The transit of 1882 will not settle the value of the solar parallax, but it will contribute to that result, directly as a trigonometrical method, and indirectly through the gravitational methods with which the final solution of the problem must rest. As our knowledge of the earth's mass may be made to depend upon quantities which continually increase with the time, it will ultimately attain great exactness, and then the solar parallax will be known with the same exactness. Long before that happy day arrives the present generation of astronomers will have passed over to the silent majority, but not without the satisfactien of knowing that their labors will contribute to that fullness of knowledge which shall be the heritage of their successors.

EPHEMERIS OF THE SATELLITES OF MARS FOR THE OPPOSITION OF 1881.*

By H. S. Pritchett.

Owing to the greater distance from the Earth and the Sun, the present opposition of Mars will not be so favorable as the two preceding ones ; still these distances will be sufficiently small to permit many useful observations of physical phenomena, and, in the case of large telescopes, observations of the satellites. In one respect, the planet is much more favorably situated than in the former oppositions referred to, since it reaches this year a declination of 26° north, and hence will be observed at a much higher altitude. Physical observations, either measures or drawings, by amateur astronomers with good glasses, if carefully made and published, will be useful when finally reduced and compared.

During the last opposition several series of micrometric measures of the diameter of the planet were made by observers with good telescopes which showed curious differences both between themselves and when compared with the results obtained from the heliometer. Some of these measures seemed to show an appreciable flattening at the poles, while others showed no such flattening. It will be interesting to have these measures repeated during the present opposition, with a careful discussion of the sources and effects of personal error.

The satellites were observed last opposition with at least one of the large reflectors, with the great refractor at Washington, with the 15-inch refractor of the Harvard College Observatory, and with the 12¼ inch refractor of the Morrison Observatory, and were seen with other instruments. Before December 1st of this year the satellites will be considerably brighter than when last observed in 1879 with the Harvard College refractor, and also brighter than when last observed with the Morrison Observatory refractor. It seems possible, therefore, that they may be seen this year with telescopes even of moderate size.

The following ephemeris (derived from the elements of Prof. A. Hall, A. N. No. 2394) has been computed at the request of several observers, and will be found convenient for any who may wish to observe these satellites. In connection with the discussion of the relative merits of reflectors and refractors, excited by the observations of these satellites, it may be interesting to many to try if they can see them.

In the case of Deimos, the outer satellite, the ephemeris gives the Washington mean times of the east and west elongations, together with the position-angle and distance at the time of elongation. In the case of Phobos only the times of western elongations are given, as the revolution time is very short and the times of eastern elongations may be obtained by a simple interpolation. The aberration time is not included in the time given, but it may be taken from the table at the end if desired, the effect of the aberration being to make the satellites about five minutes late at each elongation. The relative brightness on different days may be obtained from the same table, taking the brightness on Nov. 20 as unity. As was shown by the observations of 1879, Prof. Hall's elements are very nearly correct, so that the correction to this ephemeris will be quite small.

* Read before the St. Louis Academy of Sciences.

Deimos.

Date.	Direction of Elongation.	Wash. M. T.	Pos. Ang.	Dist.	Date.	Direction of Elongation.	Wash. M. T.	Pos. Ang.	Dist.
		H. M.					H. M.		
Dec. 5	E	16 33	-----	----	Dec. 22	E	2 2	-----	----
6	W	7 42	-----	----		W	17 10	-----	----
	E	22 50	----	----	23	E	8 19	-----	----
7	W	13 58	-----	----		W	23 26	-----	----
8	E	5 7	-----	----	24	E	14 33	-----	----
	W	20 14	250°.2	52".4	25	W	5 41	-----	----
9	E	11 23	-----	----		E	20 49	-----	----
10	W	2 31	-----	----	26	W	11 57	-----	----
	E	17 39	-----	----	27	E	3 5	-----	----
11	W	8 47	-----	----		W	18 12	-----	----
	E	23 55	-----	----	28	E	9 20	-----	----
12	W	15 3	-----	----	29	W	0 28	-----	----
13	E	6 11	-----	----		E	15 36	-----	----
	W	21 19	249°.7	53".2	30	W	6 44	-----	----
14	E	12 27	-----	----		E	21 52	-----	----
15	W	3 35	-----	----	31	W	13 0	-----	----
	E	18 43	-----	----	Jan 1	E	4 7	-----	----
16	W	9 31	-----	----		W	19 15	245°.9	52".4
17	E	0 59	-----	----	2	E	10 23	-----	----
	W	16 7	-----	----	3	W	1 31	-----	----
18	E	7 15	-----	----		E	16 39	-----	----
	W	22 23	-----	----	4	W	7 47	-----	----
19	E	13 31	-----	----		E	22 55	-----	----
20	W	4 39	248°.5	53".7	5	W	14 3	-----	----
	E	19 47	-----	----	6	E	5 11	-----	----
21	W	10 54	-----	----		W	20 19	-----	----

Phobos.

Date.	Wash. M. T.	Pos. Ang.	Dist.	Date.	Wash. M. T.	Pos. Ang.	Dist.
	H. M.				H. M.		
Dec. 2	1 40	251°.5	20".3	Dec. 20	5 51	248°.5	21".5
	9 19	------	----		13 30	------	----
	16 58	------	----		21 9	------	----
3	0 37	------	----	21	4 48	------	----
	8 16	------	----		12 27	------	----
	15 56	------	----		20 6	------	----
	23 35	------	----	22	3 46	------	----
4	7 14	------	----		11 25	------	----
	14 53	------	----		19 4	------	----
	22 32	------	----	23	2 43	------	----
5	6 12	------	----		10 22	------	----
	13 51	------	----		18 1	------	----
	21 31	------	----	24	1 40	------	----
6	5 10	------	----		9 19	------	----
	12 49	------	----		16 58	------	----
	20 28	------	----	25	0 37	------	----
7	4 7	------	----		8 16	------	----
	11 46	------	----		15 55	------	----
	19 25	250°.4	20".9		23 34	247.°3	21".4
8	3 5	------	----	26	7 13	------	----
	10 44	------	----		14 53	------	----
	18 23	------	----		22 32	------	----
9	2 2	------	----	27	6 11	------	----
	9 41	------	----		13 50	------	----
	17 20	------	----		21 29	------	----
10	0 51	------	----	28	5 8	------	----
	8 30	------	----		12 47	------	----
	16 18	------	----		20 26	------	----
	23 57	------	----	29	4 5	------	---
11	7 36	------	----		11 44	------	----
	15 15	------	----		19 23	------	----
	22 54	------	----	30	3 3	------	----
12	6 33	------	----		10 42	------	----
	14 13	------	----		18 21	------	----
	21 52	------	----	31	2 0	------	----
13	5 31	------	----		9 39	------	----
	13 10	------	----		17 18	------	----
	20 49	249°.7	21".3	Jan. 1	0 57	246.°0	21".1
14	3 28	------	----		8 36	------	----
	12 7	------	----		16 15	------	----
	19 46	------	----		23 54	------	----
15	3 25	------	----	2	7 33	------	----
	11 4	------	----		15 13	------	----
	18 43	------	----		22 52	------	----
16	2 23	------	----	3	6 31	------	----
	10 2	------	----		14 10	------	----
	17 41	------	----		21 49	------	----
17	1 20	------	----	4	5 28	------	----
	8 59	------	----		13 7	------	----
	16 38	------	----		20 46	------	----
18	0 17	------	----	5	4 25	------	----
	7 56	------	----		12 4	------	----
	15 35	------	----		19 43	------	----
	23 14	------	----	6	3 23	------	----
19	6 53	------	----		11 2	------	----
	14 33	------	----		18 41	------	----
	22 12	------	----	7	2 20	------	----

DATE.	Brightness.	Semi-diam. Mars.	Aberration Time.
			M.
Dec. 2.0	1.15	7.3	5.3
8.0	1.21	7.5	5.2
14.0	1.24	7.6	5.0
20.0	1.26	7.7	5.0
26.0	1.24	7.7	5.0
Jan. 1.0	1.18	7.6	—5.1

From this it will be seen that Phobos, even on the most favorable date, will be only about 14″ distant from the limb of the planet. In 1877 this satellite was observed with the 12¼ equatorial of the Morrison Observatory when only 7″ distant. In the present opposition the satellite will be much fainter, but on the other hand the brightness of the planet will be considerably diminished. It seems possible, therefore, that this satellite may be seen with glasses of moderate size.

WASHINGTON UNIVERSITY, *Nov.*, 1881.

ELEMENTS OF QUATERNIONS.*

BY A. S. HARDY, Ph. D., Professor of Mathematics, Dartmouth College.

The American press may be expected to teem for the next twenty-five years with elementary treatises on quaternions, and as this work of Professor Hardy's is, we believe, the first of the series, it merits on this account the more attention. The book has a quite neat and attractive exterior, and the mechanical execution is very fair, though a few defects in letter press and engraving are noticeable. The experiment of printing small Alphas with an oblique line through them seems to be a failure. See pp. 45 and 60.

We cannot think the title happily chosen. There is an incongruity, if not positive impropriety, in assigning to a scant text-book intended for beginners in the class-room a name associated these fifteen years with the great and classic work of Hamilton. This however, is a matter of taste. One of the most important and difficult steps in the logical development of the calculus of quaternions, to which their inventor gave no little attention, is that of assigning a versor power to a vector, or of representing rotation by a symbol that had hitherto been appropriated exclusively to vection or translation. This, in the book before us, is disposed of in a few lines, when, even in a treatise where brevity must be studied, it is well worthy of as many pages. There is, also, throughout the work, an unfortunate fondness for the plane, where quaternions are often at a disadvantage, and where their real power and usefulness cannot be exhibited. The author may have intended to thus avail himself of the student's greater familiarity with the geometry of the plane, while introducing him to a new method; but it ought to be borne in mind that one of the chief claims quaternions have on the teacher of geometry is that they are specially fitted to free the student from the too prevalent restriction of his conceptions to two dimensions. A curious example of this tendency of the book is afforded near the end in applications to loci. Here the author systematically interprets equations as relating to the conic sections, when in reality they frequently relate to quadrics of revolution, the restriction to plane loci having been eliminated in the process of their formation; and when he comes "to transform the proceeding equations into the usual cartesian forms," instead of substituting a trinomial for the variable vector, he imposes a restriction to two dimensions by adopting a binomial, and of course comes out with a plane section in place of the surface itself. Notwithstanding these imperfections, Prof. Hardy has evidently studied his subject and written his book with some care, and with a view to the requirements and opportunities of those for whom it is intended, and it will doubtless prove useful as an introduction to quaternions.

ALEX. S. CHRISTIE.

U. S. COAST & GEODETIC SURVEY,
WASHINGTON, *November* 11, 1881.

* 8°, pp. VIII, 230. Boston, Ginn, Heath & Co., 1881.

LARGE TELESCOPES.

PROFESSOR EDWARD C. PICKERING makes the following suggestion in regard to mounting a telescope on a new plan. He says:—"The small amount of work accomplished with large telescopes has often been the subject of unfavorable comment. This criticism applies with especial force in America, where there are nearly a dozen telescopes having an aperture of a foot or over, besides two of the largest size now in course of construction, and two of twenty-six and twenty-four inches aperture which are unmounted and have been for several years perfectly useless. Among so many it seems as if one might be spared for a trial of the following plan, which, if successful, would produce at a small expense far more work than could be obtained with a mounting of the usual form.

Suppose that the telescope is placed horizontally at right angles to the meridian, and that a plane reflector inclined to its axis by 45° is placed in front of it. This reflector may revolve around an axis coinciding with that of the telescope. Such a mounting has been used in transit instruments, and gives much satisfaction in the meridian photometer of the Harvard College Observatory. The principal difficulty with a large instrument would lie in the flexure of the reflector. This difficulty has, however, been overcome in a great measure in reflecting telescopes by various ingenious devices. In the present case, since the reflector rotates only around one axis instead of two, the problem is much simplified. A slight motion at right angles of perhaps 5° would be a great convenience, as will be shown below, and would probably be insufficient to materially affect the flexure. It may be said that it is more difficult to make a plane surface than one that is curved. But the principal effect of a slight curvature would be to change the focus of the telescope, the aberration being much less than the effect of the varying flexure. Let us admit, however, that the best definition cannot be obtained, in considering the purposes to which such an instrument could be applied without disadvantage.

Many advantages will be apparent on comparing such a mounting with an equatorial. Great steadiness would be secured, since the mirror would be the only portion moved, and this would be placed directly upon a low pier. Instead of a large and expensive dome which is moved with difficulty, the mirror would be protected by a small shed, of which the roof could be easily removed. It would therefore be opened and ready for use in a very short time, and would quickly take the temperature of the surrounding air. The object-glass would be mounted directly upon a second pier, and, as it would not be moved, would be in very little danger of accident. The tube could be made of tin or other inexpensive material, as its flexure is of no importance. It could easily be protected from the changes of the temperature so troublesome in the tube of a large equatorial. If preferred it might even be exhausted of air, or filled with hydrogen, and the effect of the changes of temperature thus greatly reduced.

The eyepiece could be mounted on a third pier, and would be so far distant horizontally from the mirror and object-glass that there is no reason that it should not be inclosed in a room which may be warmed. The comfort in winter of working in a warm room will be appreciated by those who have used a large telescope in a cold climate. The result is sure to be an increased precision in

the observations, and a possibility of prolonging them over longer intervals. A similar effect is produced by the constant direction of the line of sight. No especial observing chair is needed. There is no limit to the size of the attachments which may be made to the eyepiece, since they need not be moved. This is a great advantage in certain spectroscopic and photometric measurements. A strong wind interferes seriously with many observations, as it is impossible to make a telescope so stiff that it will not be shaken by sudden gusts. In the plan here proposed the mirror alone is exposed, and its surface is too small to give trouble.

By means of a long handle the position of the mirror may be regulated from the eye-end, and the declination of the object observed read by small telescopes. If the mirror can be moved at right angles to the meridian 5° from its central position, an object at the equator may be followed for forty minutes, and other objects for a longer period. Without this motion an object may be followed for three or four minutes by moving the eyepiece alone. Clockwork may be applied to the mirror, or less easily to the eyepiece. The focal length may be increased almost indefinitely if desired, and certain advantages will be thus attained in the diminution in the defects of the object-glass, although those of the reflector will not be affected. If the telescope is to be erected at a great elevation the advantages of the present plan are at once apparent. Many nights of observation would be secured which otherwise would be lost owing to the wind and cold. The simplicity in the construction of the building would be a great advantage, as a large dome in so exposed a situation would be kept free from snow with much difficulty, and might be a source of danger in winter storms. If found impracticable to observe during the winter, it would be possible to have a duplicate mounting below, and remove the lens and mirror from one to the other.

It is evident that the saving of cost would be very great, not only in the observatory building aud dome, but in the tube, observing chair, clockwork, &c.

If a reflector could be constructed whose surface was the portion of a paraboloid whose abscissa equalled that of the focus, the instrument could be much simplified. No object-glass would then be required, the reflector taking the place both of mirror and lens. All the light intercepted by the objective would thus be saved, and but a single surface need be adjusted and corrected. With the advance in mechanical methods this does not seem wholly impracticable, especially with a mirror of long focus. Since the final correction must always be made by hand, it makes but little difference what is the exact form of the surface.

In any case it would be a great advantage that the mirror could be reground, repolished, or resilvered without moving it from its place. It would only be necessary to place it horizontally, and the grinding machinery could be kept permanently near it. If plane, the perfection of its form could also be tested at any time by setting it on edge, and viewing the image it reflected by a collimating eyepiece attached to the large telescope. Another method would be to place a heliotrope a few hundred yards to the north or south of it, and the light from this would form an excellent artificial star, available whenever the sun shone.

The greatest advantage is the rapidity with which observations could be made. No more time would be lost in identification than with a transit instrument, so that a large number of objects could be examined in the course of a single hour. Any one who has worked with a large telescope knows how much time is lost in opening and closing the dome and in finding and identifying minute objects.

Let us now consider to what purposes a large telescope mounted as suggested might be applied.

1. Sweeping. For the discovery of new objects this mounting presents especial advantages. It might be used for the detection of new double stars, of nebulæ, of red stars, or of objects having singular spectra, as planetary nebulæ, banded stars, and variables of long period. Suppose that the field of view had a diameter of somewhat over one minute of time, and that a small motor was attached to the mirror which would move it uniformly over 5° in declination in this time, and then bring it quickly back to its first position. The observer would then have presented to him a series of zones 5° long and one minute wide. The sweeps should overlap by a small amount, so that the entire region could be covered in a single evening. The observer could have a few seconds rest between each zone, while the motion of the mirror was reversed. If an object of interest was suspected, it could be located by merely noting the time at which it was seen. The right ascension would be given directly, and the declination would be found by interpolation from the time of beginning and ending the sweep. An examination of the object and a determination of its exact location should be made on another evening.

2. Measures of position. For many purposes positions could be determined with this instrument as in a transit circle. It would generally be better, however, to make the measures differential, leaving the mirror at rest and observing the transits of the object to be determined and of two or more companion stars. The method of the ring micrometer might be employed, or some modification of that with inclined lines. In the latter case the zero of position could be found by the transit of preceding stars, by setting the reticule by a divided position circle, or perhaps better by keeping it in a fixed position, determining the direction of the lines once for all, and applying a correction for the declination of the object observed. Stars could be compared differing nearly a degree in declination, as the eyepiece could be moved without danger of disturbing the reticule. For the same reason the star could be followed for three or four minutes, and its transit over a great number of wires observed. It is here assumed that the distortion produced by the mirror is not very great. A slight distortion would do little harm, as it would be the same for all stars of equal brightness. If the stars differ greatly in brightness, the observer should determine his personal equation between them in any case, and the same operation would eliminate the effect of the distortion. The large aperture of the instrument would permit the observation of stars quite beyond the reach of any meridian circle. The faintest asteroids could thus be readily measured, and could probably be followed in many cases on successive evenings to their stationary points. Zones of stars could be observed very conveniently for the formation of charts or catalogues, for the discovery of asteroids, stars with large proper motion, &c.

Probably the definition could not be sufficiently good for the measurement of the closer double stars, but if clockwork was attached, faint companions could be measured, or approximate positions of the coarser pairs determined very rapidly. The positions of nebulæ could also be observed with a view to detecting their proper motion. Stars having a large proper motion might be observed, and the observations so arranged that any very large parallax would be detected. A similar search for a large parallax of variable stars, short-period binaries, minute planetary nebulæ, or stars having singular spectra, might lead to interesting results. The argument that no ordinary star is very near does not apply to such objects.

3. Spectroscopy. The increased dimensions which could be given to the spectroscope, and its steadiness, would compensate in a great measure for a defect in definition. By Zöllner's reversion spectroscope the slit might be dispensed with, and also the necessity of clockwork. So many stars could be observed in a single evening that systematic errors could be in a great measure eliminated, and as the spectroscope would not be moved, we should have a great assurance that the deviations were

real. Of the 6000 nebulæ hitherto discovered we know nothing of the spectrum of more than 300 or 400, while the observation of all the others with a large horizontal telescope would not be a very formidable undertaking. It would also be interesting to observe the spectra of all the clusters. It is possible that some may consist of stars having singular spectra, or even of disconnected nebulous masses, in fact forming clusters of planetary nebulæ. The interesting discovery by Dr. Copeland that Burnham's double nebula in Cygnus is gaseous, shows the same tendency to aggregation in these bodies as in stars. Observations of the spectra of all the red stars and variables would also probably lead to interesting results.

4. Photometry. Should the instrument be devoted to photometry numerous problems suggest themselves. Variable stars could be observed near their minimum when too faint to be identified with an equatorial without great loss of time. Faint stars in zones or faint companions to bright stars could be measured very rapidly. The relative light of all the asteroids would be an interesting problem. Many coarse clusters appear to consist of stars of nearly equal brightness. Their light compared with their distances apart might aid our study of their formation. Another useful investigation would be to measure the brightness of all the nebulæ.

In the application of physics to astronomy doubtless many other problems will suggest themselves. Thus no satisfactory results have been obtained in the attempt to measure the heat of the stars with the tasimeter. The use of this instrument would be vastly simplified if it was placed on a solid pier near the ground, was not moved during the observation, and could be perfectly protected from other changes of temperature than those which it was intended to measure.

As either of the problems proposed above would occupy the time of a telescope for at least one year, it is obvious that there could be no difficulty in keeping such an instrument occupied indefinitely.

The horizontal mounting is especially adapted to an elevated position, and would permit the use of a telescope where an equatorial mounting would be quite impracticable. On the other hand, to an amateur, or for purposes of instruction, an instrument which could be set quickly from one object to another, and where the observers need not be exposed to the cold, would offer many advantages. The impossibility of observing far from the meridian would be less important with a large instrument, where the number of objects to select from is very great.

There are certain purposes to which this mounting could not be advantageously applied. The study of close double stars and other objects requiring long examination and very perfect definition could be better left to other instruments. The sun, moon, and planets can also generally be better observed off the meridian. If, however, the entire time of an instrument can be employed to advantage, and it can collect several times as much material as an instrument of the usual form, it is no evidence against its trial that there are certain problems to which it cannot be advantageously applied.

The working force required for such an instrument should consist of at least one observer, an assistant to record, and a number of copyists and computers to prepare the working lists, reduce the observations, prepare them for the press, and read and check the proof-sheets. A large volume of valuable observations could thus be produced every year, which would require at least double the time and money to produce by the same telescope mounted equatorially. The difference in the amount of work will be evident when we compare the number of objects observed with a transit instrument per night, with those observed with an equatorial. A hundred objects in various declinations might be examined in a single evening, while it is seldom that the same number could be identified and measured by an equatorial in a week.

ON MAXIMUM SYNCHRONOUS GLACIATION.*

By W. J. McGee.

In the development of knowledge of the cosmos, the tendency has ever been to look at first upon all phenomena as mystical and incomprehensible; and only after repeated observation and much study has it been decided that any class of phenomena may be the result of the operation of the identical laws whose existence is established by every-day observation. Thus, in geology, catastrophism prevailed long, but finally yielded to a rational uniformitarianism; in general biology the idea of special creation has only given way to that of derivation within the memory of a child; and in anthropology the mystical view yet generally prevails. The narrow domain of glacial physics, as embodied in the glacial theory, is still in the transitional stage. When that theory was first acceptably propounded by Agassiz, the details were so varied, the recognized relations so unique, and the whole conception so grand and startling, that even the more conservative of those who early became its advocates, forgot for the time the necessity for keeping all assumed data within the bounds of actual observation or legitimate induction; and hence not the least valuable of the later contributions to the theory are those which bring out its relation to established laws. Such is the aim of the memoirs bearing the above title; the particular phase of the subject discussed being that known as the "ice-cap theory."

The conclusion of Tyndall that such a supply of heat as may be necessary to produce large quantities of aqueous vapor, and an area of sufficiently low temperature to not only condense but congeal the vapor brought to it, are the first requisites for glaciation, is adopted at the outset; but it is shown that while the regions which furnish and those which congeal the vapor may be contiguous, they must be quite distinct. There is no other substance than water in the solid state which will abstract heat from the superfluitant vapor with such facility as to form, when spread over the surface, a condenser of sufficient power to meet the requirement of glaciation; and such a condenser must so far exceed in capacity any tax that may ever be placed upon it, that it will *immediately* condense and congeal all moisture that may be brought to it by aërial currents; for if the vapor is not immediately condensed it will cut off radiation from the ice below, and thus accelerate melting; and if the vapor is only condensed but not congealed it will fall as rain, and every pound of it will melt 143 pounds of ice before it is itself frozen. *The integrity of the condenser hence depends on its capacity being far in excess of the work it may be called upon to perform.* Now if a condenser formed of an ice-sheet 1,200 or 1,400 miles in diameter on any part of the globe be assumed, it is manifest that the tendency of the accumulating ice will be to form an annular belt of maximum thickness, gradually attenuating toward the center of the area; for if the vapor-laden air were not immediately robbed of its moisture in sweeping over the condenser, the marginal portions of the ice would soon be destroyed. But no matter how perfect the condenser, glaciation can never occur unless there are ample quantities of vapor supplied to it; and the greatest possible accumulation of ice at any latitude may accordingly be regarded as proportional to the moisture conveyed thither. It follows that the greatest possible accumulation of ice in polar regions can never have been nearly as vast as that at lower latitudes during the quaternary; and indeed it was probably never much greater than at present. Geological evidence, so far as accessible, corroborates this view.

Similar conclusions are reached by an independent line of investigation. Within an extensive area covered by ice or snow, both aërial and aqueous currents would be either stopped or so modified as to be practically inopera-

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tive as distributors of heat. The temperature would hence become approximately proportional to the solar accession, which has been computed by Much for the various latitudes, and may be roughly reduced to thermometrical degrees by means of an easily determined constant. Moreover the presence of the ice would greatly facilitate both radiation and direct reflection of solar energy. The general diminution of temperature produced in this manner is calculated for each latitude; and from a comparison of these figures with actual temperatures, as recorded by Dove, the temperature of the whole hemisphere when the ice-sheet extends to any latitude is also computed. From the several figures obtained it appears that if the globe were encrusted with ice, the crust would probably (and indeed almost certainly) never be melted unless by proper terrestrial heat; while the temperature in polar regions, as well as over much of the ice-covered hemisphere, would sink so low as to practically eliminate all aqueous vapor and effectually prevent the further accumulation of ice. The annual variations in solar intensity would not materially affect the values obtained.

Since the results reached in the manner indicated embody values widely different from those of existing temperatures, and are hence *a priori* improbable, a detailed investigation of certain meteorological phenomena is undertaken in order to verify these results. The observed and computed temperatures of the northern hemisphere are first compared, and are found to indicate that the temperature-equalizing agencies are 1.5 times as effective in summer as in winter. The effect of atmospheric dryness in diminishing the effectiveness of these agencies is then found to be still greater. The values developed in the investigation of this subject demonstrate that the climatal perturbations previously pointed out as the necessary result of the considerable extension of a polar ice-sheet do not differ in kind, but only in degree, from those whose constant occurrence is a matter of authoritative record; and analogy with observed phenomena moreover indicates that the calculated extent of these vicissitudes is in perfect harmony with the magnitude of the formulated course.

The figures obtained incidentally demonstrate the existence of an empirical meteorological law, which may be stated as follows: *Any increase in thermometrical range is accompanied by a diminution in mean temperature.* Since the law strongly corroborates the results reached by the second line of investigation, it is quite fully considered, especially in its application to the present eondition of the two hemispheres. That hemisphere whose winters occur in aphelion experiences a greater variation in solar accession and consequently in temperature than the opposite one, and hence, according to the law, ought to have a lower mean annual temperature. The southern hemisphere is so situated at present; and accordingly, notwithstanding more favorable geographical and other conditions, its temperature is lower than that of the northern hemisphere. The bearing of the law on Croll's celebrated theory of secular variations in terrestrial climate is manifest.

Since it is developed in both lines of investigation that the accumulation of glacier ice is dependent upon, and in a general way proportional with, precipitation, the maximum accumulation at any latitude may be roughly computed. The final determination is as follows:

Latitude 40°	18,594 feet.
" 50	9,777 "
" 60	5,728 "
" 70	2,800 "
" 80	1,799 "
" 90	1,440 "

It may accordingly be concluded that a sufficient accumulation of polar ice to displace seriously the earth's center of gravity, or to influence the motion of middle-latitude glaciers, never can have taken place.

The nature and course of ice motion are discussed at some length; and the phenomenon is shown to be analogous to those exhibited by all classes of substances, though generally in a less striking degree. The "viscous theory" of Forbes is adopted with some modifications; and the principal objections thereto are considered. It is also pointed out that ice-streams are necessarily in tension, and hence that the central mass of an ice-field can exercise an influence on the motion of its peripheral portions. The assumption of a vast polar ice-cap to explain the motion of the quaternary glaciers accordingly appears to be not only unnecessary but incompetent.

GLUCOSE.

By Albert E. Ebert, Peoria, Ill.

The process of making glucose, or grape sugar, is as follows: corn, after being shelled, is placed in large tubs and soaked in hot water from a day and a half to five days, or even longer, the time depending on the hardness of the grain. If fermentation is not wished, the water is changed when the substance begins to sour. It is then ground, while wet, with ordinary burr stones, and with a stream of water running into the hopper with the corn. The meal, or "chop," is then run on vibrating sieves, made of fine silk bolting cloth, also fed with streams of water. By this treatment the starch, which washes through the sieves, is separated from the gluten and cellular matter, which waste portions go over the tail of the sieves, and after passing through rollers which squeeze the mass, and return the water to the sieves, is sold for feed. The portion which went through the sieves is run into tanks and settled, the water drawn off and the sediment again mixed with clean water and treated with alkali, about one pound of caustic soda, (more or less, according to the hardness of the water), being used for each bushel of corn. This treatment separates any traces of gluten from the starch, which is then run into metal-lined troughs or gutters about eight inches deep, from fifteen to thirty-six inches wide, and usually from one hundred to one hundred and fifty feet long. These are inclined slightly, and the water runs off at the lower end, leaving the starch as a sediment at the bottom. In some factories this starch mixture goes direct from the sieves to the gutters or "tables," as they are usually called. It is left to dry somewhat in the tables, and is then shoveled out. At this stage of the process it is known as "green starch." It is quite solid, but moist, containing about fifty per cent. of water.

Up to this point the process is the same as starch-making. Starchmakers take the green starch and wash it, some several times, by mixing it with clean water and allowing it to settle, then drawing off the water, and repeating the process. It is then sometimes bleached by chloride of lime or sulphurous acid, and after washing, settled, made into blocks about eight inches square, when it is dried in a kiln. For the finer grades, about half an inch of each side of the cake is shaved off when partially dry, the rest of the cake being wrapped in paper and put back into the kiln until it forms into little sticks or pipes.

For glucose, however, the green starch is made quite thin with water, and run into converters, usually after several additional washings. The converters are large wood tanks or tubs, where it is treated with acids, sulphuric being usually used, although muriatic, nitric, or even oxalic may be substituted. Sulphuric is preferred, as it is cheap and easily gotten rid of in an after stage of the process, when it has performed its work. The acid does not combine with the starch, but merely exerts a catalytic action; therefore the necessity of providing for its removal. While under the acid treatment the contents of the converters are heated to the boiling point by

means of steam pipes coiled inside the tubs, or by steam jets. Some use pressure converters, which are iron or copper tanks like a boiler, when the conversion is much quicker. The operator makes frequent chemical tests to determine when the starch is entirely converted into sugar, and when this is accomplished the mixture is drawn into another vat where the acid is neutralized with some form of carbonate of lime, as marble dust, chalk or whiting. The liquid is sometimes bleached by the use of sulphurous acid at this stage of manufacture. It is now a very dilute solution of glucose, and besides incidental impurities, contains sulphate of lime formed by the action of the sulphuric acid on the carbonate, and whatever carbonate of lime was used in excess of the sulphuric acid present. These are separated by straining through cloth or bag filters and afterward percolating through columns of bone charcoal, eight or ten feet deep. When decolorized, it is drawn into the "vacuum pan," which is a large, strong tank of iron or copper, with steam pipes coiled inside for heating, and from which the air is partially exhausted by an air pump, and in which the syrup is boiled down at a temperature of 100° to to 145°. When concentrated to a specific gravity of about 1400 it is drawn off and again strained or filtered, and is ready for the market as glucose, this being the commercial term for the syrup only. The term grape sugar is applied to the dry glucose, and this is produced by carrying the conversion further before neutralization.

The syrup is used, principally, for mixing with dark colored cane syrup for making light colored table syrups (nearly all the table syrups now sold contain it, and frequently from 75 per cent. to even a larger quantity), and also in making wine, ale, beer and vinegar. On a smaller scale it is used in tobacco manufacture, the adulteration of honey, fruit preserving, etc. Both the solid and liquid forms are largely used in candy making, for which it has several marked advantages. A syrup is prepared expressly for this use, in which the conversion of the starch into sugar is only partial, the syrup containing, of its solid matter, about eighty per cent. of the intermediate product, dextrin, and twenty of glucose. The large consumers of glucose require slightly different syrups. Wine growers, for instance, use a syrup free from dextrin. Brewers desire a very small proportion of it, to give body to the beer, while vinegar makers use a syrup free from gum. The dry glucose, or grape sugar, seems, aside from its legitimate use in candy making, to be most largely in demand for the adulteration of cane sugar. No objections, save of a moral and financial nature, can be urged against this, but it is well to remember that for its value as a sweetener, compared with cane sugar at ten cents per pound, glucose is worth but four cents. So much has been written against the manufacture of glucose, on account of its use as an adulterant of cane sugar, that it is, perhaps, only just to say that it is certainly the least objectionable of any of the articles used for that purpose. It is perfectly wholesome, being in fact the physiological sugar, and has about two-fifths the sweetening power of cane sugar, which is more than can be said of terra alba, starch, bone dust, sand, etc., while its most probable impurity, calcium sulphate, can, from its insolubility, be present only in minute quantity, probably not more largely than in most potable waters, and is not in any sense noxious.—*The Druggist.*

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING NOV. 19, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

NOVEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.
Sunday, 13..	29.636	29.790	12 p. m.	29.542	1 a. m.
Monday, 14..	29.937	30.002	9 p. m.	29.790	0 a. m.
Tuesday, 15..	30.214	30.442	12 p. m.	29.976	1 a. m.
Wednesday, 16.	30.500	30.550	9 a. m.	30.442	0 a. m.
Thursday, 17..	30.327	30.464	0 a. m.	30.138	12 p. m.
Friday, 18..	29.869	30.138	0 a. m.	29.690	12 p. m.
Saturday, 19..	29.669	29.798	12 p. m.	29.600	1 p. m.

NOVEMBER.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun
Sunday, 13..	51.6	49.0	59	0 a. m.	58	0 a. m.	42	12 p. m.	42	12 p. m.	115.
Monday, 14..	46.3	43.6	53	2 p. m.	48	2 p. m.	40	5 a. m.	40	5 a. m.	106.
Tuesday, 15..	41.3	38.3	46	4 a. m.	43	3 a. m.	36	8 a. m.	34	8 a. m.	104.
Wednesday, 16.	39.7	38.0	45	4 p. m.	41	4 p. m.	33	6 a. m.	33	6 a. m.	101.
Thursday, 17..	47.6	45.7	55	3 p. m.	51	4 p. m.	37	8 a. m.	37	8 a. m.	110.
Friday, 18..	58.3	55.6	61	2 p. m.	58	12 p. m.	52	0 a. m.	50	0 a. m.	82.
Saturday, 19..	50.3	49.3	61	0 a. m.	58	0 a. m.	45	12 p. m.	43	12 p. m.	62.

Mean for the week .... 30.021 inches.
Maximum for the week at 9 a. m., Nov. 16th .... 30.550 "
Minimum " at 1 a. m., Nov. 13th .... 29.542 "
Range .... 1.008 "

	Dry.		Wet.	
Mean for the week	47.8 degrees		45.6 degrees.	
Maximum for the week, at 2 pm. 18th	61.	" at 12 pm 18th,	58.	"
Minimum " " 6 am. 16th	33.	" at 6 am 16th,	33.	"
Range " " ---- ----	28.	" ------------	25.	"

NOVEMBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.
Sunday, 13.	w. s. w.	w. n. w.	w. n. w.	241	7	3.00 pm	.389	.282	.275	93	62	92
Monday, 14.	w. s. w.	w.	e. n. e.	197	7¼	3.30 pm	.235	.269	.251	91	66	84
Tuesday, 15.	w. n. w.	n. n. w.	n. w.	369	19¼	7.30 am	.190	.186	.203	74	67	82
Wednesday, 16.	n. w.	w. s. w.	w.	143	1	2.00 pm	.188	.208	.231	100	75	83
Thursday, 17.	s.	s.	s. s. w.	170	4¾	9.30 pm	.229	.295	.334	100	73	86
Friday, 18.	s. w.	w. s. w.	s. s. w.	258	5¾	11 15 am	.362	.412	.456	86	77	88
Saturday, 19.	n. n. e.	n.	n. w.	118	6¾	8.00 pm	.335	.374	.309	92	100	85

NOVEMBER.	CLOUDS. Clear, 0 Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 13.	0	1 cir. s.	0	-----	----	-----	--	0
Monday, 14.	0	7 cir. cu.	0	-----	-----	-----	--	5
Tuesday, 15.	7 cu.	4 cu.	0	-----	-----	-----	--	2
Wednesday, 16.	0	0	0	-----	-----	-----	--	0
Thursday, 17.	0	1 s.	7 cu.	-----	-----	-----	--	2
Friday, 18.	9 cu.	9 cu.	10	-----	-----	-----	--	0
Saturday, 19.	9 cu.	9 cu.	10	5.15 pm	9 pm	3.45	.03	3

Distance traveled during the week .... 1,496 miles.
Maximum force .... 19¼ lbs.

Total amount of water for the week .... 0.05 inch.
Duration of rain .... 3 hours, 45 minutes

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